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viduals may continue to molt in the spring, while others of the same species cease to do so.

V. The remiges are molted less frequently than any other part of the plumage. As a rule, they are only renewed at the annual molt (exception Dolichonyx).

VI. Variability in the order of molt in the remiges and presence or absence of molt in the flight feathers at the end of the first summer are generally family characters, i. e., Cerylе differs from any other species treated of in this paper in the order of molt in the primaries. All Picidæ and all Icteridæ, except Icterus (and Dolichonyx ?), molt the flight feathers with the rest of the first plumage. None of the Oscines except Icteridæ (as above), some (all ?) Hirundinidæ, Olocoris and Cardinalis molt the flight feathers at this time.

Mr. Stone's conclusions as to "color-change without moulting" are the same as those reached by Chapman, in his article on "The Changes of Plumage in the Dunlin and Sanderling," namely: that color-change without molt or abrasion is incapable of taking place from the very nature of the structure of a feather, and that all the cases so reported can be otherwise acconnted for. (Proceeds. Acad. Nat. Sciences, Phila., 1896.)

The Florida Deer.—The fact that the Florida deer is but little more than half the size of the deer of northeastern United States, together with certain cranial and dental peculiarities, is sufficient, according to Mr. Outram Bangs, to give it full specific rank. He therefore describes it under the name *Cariacus osceola.* The most striking differences between the Florida animal and its northern relatives are (1) the shape and size of the nasal and maxillary bones, and (2) the very large molar and premolar teeth. (Proceeds. Biol. Soc. Washington, Vol. X, 1896.)

ENTOMOLOGY.[1]

Professor Forbes' Eighth Report.—The nineteenth report from the office of the State Entomologist of Illinois, covering the years 1893–4, has recently been issued. It is the eight report of the present incumbent, Professor S. A. Forbes, and adheres closely to the lines of thorough and accurate record, which have made its seven predecessors notable in the literature of economic entomology. The bulk of the volume (189 pages) is devoted to the Chinch Bug—the arch-enemy of Illinois agriculture, a voluminous record being made of the experiments with contagious diseases carried on by the entomologist and his assistants. There is also an article on the White Ant in Illinois, and in an appendix of 65 pages Mr. W. G. Johnson, assistant entomologist, gives an excellent discussion of the Mediterranean Flour Moth.

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[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

Flies Riding on Beetle's Back.—Rev. A. E. Eaton, the well-known British entomologist, writing from Bône, Algeria, sends this interesting note to the *Entomologist's Monthly Magazine*: "Across the mouth of the Seybouse, on sandy pasture land bordering the seashore, big coprophagous beetles are common, sheltering in large holes in the soil when at rest, and running about on business. A small species of *Borborinæ* may often be seen riding on their backs, chiefly on the pronotum, and about the bases of the elytra—sometimes half a dozen females on one beetle. The beetles occasionally throw themselves on their backs to try and get rid of them by rolling; but the flies elude all their efforts to dislodge them, dodging out of harm's way into the joinings of the thorax and out again, and darting from back to breast and back again, in a way that drives the beetle nearly mad. In vain she scrapes over them with her legs; in vain does she roll over or delve down amongst the roots of the herbage; the flies are as active as monkeys, and there is no shaking them off. It is difficult to get them off into the killing bottle; nothing persuades them to fly; and they would very much rather stick to the beetle than be driven off it down into the tube."

Proteid Digesting Saliva in Insect Larvæ.—Dr. Wilibald Nagel describes[2] the method of feeding in larvæ of *Dytiscus.* In these larvæ the mouth is very much reduced in size, and the ingestion of food is performed by means of suction through the much modified mandibles, the process being facilitated by the powerful digestive action of the saliva. Under natural conditions the larvæ eat only living animals, but in captivity they will also take pieces of meat. The saliva has a marked poisonous action, killing other insects, and even tadpoles of twice the size of attacking larvæ, very rapidly. The larvæ not only suck the blood of their victims, but absorb the proteid substances. Drops of salivary juice seem to paralize the victim and to ferment the proteids. The secretion is neutral, the digestion tryptic. Similar extra-oral digestion seems to occur in larvæ of ant-lions, etc., and

[2] Biol. Centralbl., XVI, 1896, 51–57, 103–112.

spiders, and according to Krause, in Cephalopods.—*Journ. Royal Micros. Society.*

Weismann on Dimorphism in Butterflies.—For some time *The Entomologist* has been publishing a series of interesting articles by Dr. August Weismann on the Seasonal Dimorphism of Lepidoptera. The June number contains a recapitulation from which we take this extract: "Although I am far from considering the few experiments, which I could here put forward, as sufficient for reaching a decisive settlement of our opinions on seasonal dimorphism, yet I cannot forbear arranging them, provisionally at least, in reference to our general conceptions of the subject. When, in the year 1875, I first set about investigating the ways of this striking and yet so long neglected phenomenon, I assumed that it was to a certain extent obvious, that this kind of dimorphism was everywhere a direct result of the various direct influences of climate, principally of the temperature, as it effects in regular alternation the spring and the summer brood of many-brooded species. I had also well considered the other possibility, that dimorphism connected with the time of the year might also depend upon the *indirect* influence of the changing environment, *i. e.*, that it might depend upon the *adaptation* to the varying environment of the butterfly according to the time of year."

I then said : " It is not inconceivable in itself, that phenomena occur among the Lepidoptera analogous to the winter and summer clothing of Alpine and Arctic mammalia and birds, only with the difference, that the change in coloring does not arise in one and the same generation, but alternately in different ones." But, at that time the fact that the *upper side* of butterflies, which is usually not adaptive, can be very variable just in summer and spring, sometimes more so than the adaptive under side, appeared to me to contradict this adaptation of seasonal dimorphism. Yet, it was the fact, that the one or the other seasonal form could be produced artificially by the operation of a higher or lower temperature, *i. e.* the stamp of the winter form might be impressed on the summer brood, and *vice versa.* I therefore concluded that it was the measure of heat which was acting during the pupal period which directly formed the species in one way or the other; and I felt the more justified in so doing, as the climatic varieties form a parallel to the seasonal forms, and as the former must, without doubt, be referred to the direct influence of climate, especially of temperature.

Thus, for example, *Chrysophanus phlœas* is seasonably dimorphic in Sardinia and at Naples; the summer form, which develops during the summer heat, is very dark, almost black, but the spring form corresponds with our German red-golden *phlœas.*

Although to-day I still look upon this view as correct, and a directly altering effect of temperature as proved, yet I have gradually been convinced, that this is not the sole origin of seasonally dimorphic variablity, but that there is also *adaptive seasonal dimorphism.* We must, I believe, distinguish *direct* and *adaptive* seasonal dimorphism ; and, I see in this distinction an important advance, which, before all, places us in position to explain the results of the various experiments undertaken by myself and others in a much more satisfactory manner.

I have already pronounced this view in a lecture delivered at Oxford in the beginning of 1894, and I have sought to show that adaptive seasonal dimorphism, which I had previously only put forward as possible, does *actually* occur. The example there given for perfect insects was, indeed, only a hypothetical one, *viz.*, the case of *Vanessa prorsa-levana;* but for larvæ, at least, I can select an example from Edward's excellent work on the North American butterflies with tolerable certainty, *viz.*, that of *Lycœna pseudargiolus,* which will be more accurately discussed later on. I did not then know what I learnt shortly afterwards from an interesting little pamphlet of Dr. G. Brandes, that cases of seasonal dimorphism had been known for a long time among *tropical* butterflies, and that among these, at least, one of the seasonal forms depend upon the assumption of a special protective coloring. Brandes maintains, with justice, that the view hitherto widely held among us is erroneous, according to which seasonal dimorphism was not to be expected in trophical countries, since the alternation of seasons is absent there. Periods of rain and drought, at least for many tropical countries, form such an alternation very sharply. At any rate, Doherty, and, somewhat later, de Nicéville, have pointed out, for Indian butterflies, a series of seasonally dimorphic species, not merely by the observation of the alternation of the two forms in nature, but by rearing the one form from the eggs of the other; thus among Satyridæe of the genera *Yphthima, Mycalesis,* and *Melanitis,* and for the species of *Junonia,* it is accepted as proved; and in all these cases the difference between the two forms principally consists in the fact that the one form seems like a dry leaf on the under side, while the other possesses another marking, and at the same time a number of ocelli.

Without engaging in the controversy as to the biological value of these ocelli, I do not for a moment doubt but that the coloring with ocelli is also an adaptive form, possibly protective or intimidating coloring. If one of the two forms had no biological significance, it could no longer exist; the single adaptive one would have replaced it. But it is obvious that the appearance of complicated details of marking and color, such as ocelli are, cannot be simply the direct effect of heat or cold, drought or humidity. *These influences are not the actual causes of such formations, but only the stimulus, which sets their primary constituents free, i. e.,* induces their development, as I tried to demonstrate in the lecture above noted. As the sufficient cause of the sleep of the marmots does not lie in the cold, but in the organization of the animal which is adapted to the cold, and as the cold only brings the existing predisposition to winter sleep into play, so among these butterflies with adaptive seasonal dimorphism the display of the one or the other marking is apparently connected, partially, at least, with one of the above named outward influences, although in reference to these trophical butterflies we do not yet know to which of them.

We recognize *temperature as the stimulus to development* with the cases of seasonal dimorphism of our indigenous butterflies, as in all cases of seasonal dimorphism, which have hitherto proved experimentally, it is always high and low temperature which gives the outward impulse to the appearance of the one or the other form where this impulse did not come exclusively from *within.*

There are, therefore, two different sources of the appearance of seasonal dimorphism: on the one hand, the *direct action* of alternating external influences, *viz.:* temperature, can bring about this change in the outward appearance; and on the other hand, the processes of selection. It is therefore necessary to consider these two kinds of seasonal dimorphism separately. It will certainly not always be easy to decide between them when a particular case has to be dealt with, as at present it is not always possible to say whether a coloring or marking has a definite biological value or not. Both causes also may co-operate in in one species.

Note on the Classification of Diplopoda.—The admitted impossibility of formulating a generally satisfactory definition of the term species exists partly because systematists have used it in the greatest variety of applications, and partly because natural groups are so diverse in structure and developmental history that a scheme calculated to elucidate one may increase confusion in another. It is hence desirable in proposing or making use of a classification to recognize as clearly as possible the conceptions under which the arrangement into the various categories of natural groups has been made.

The structure and distribution of the Diplopoda make it advantageous and usually easy to arrange them into species, which are groups of very similar individuals not connected by intermediate individuals with other groups different in details of structure, form or color. An apparent and probably sufficient cause for this is the close similarity of all Diplopoda in life-histories, habits and food. All are scavengers, able to subsist upon a variety of decaying vegetable, or even animal matter, and there has been scarcely any response to calls for special adaptations to life as parasites, commensals, or under other changed conditions. The species of Diplopoda are not only extremely local in distribution, but are generally confined to almost identical habitats, removed from which they do not long survive.

Supposing the Diplopoda to be a natural group descended from a common ancestor, we are compelled to believe that such differences as appear among them are the result of accumulated variation not greatly influenced by external selective causes. Hence, existing differences indicate in general much more remote developmental divergence than in groups which have entered more thoroughly into the struggle for existence by responding to the demands of varied conditions. In this respect the Diplopoda offer a most striking contrast to the Hexapoda, and the results are in accordance; there are more millions of species of Hexapoda than there are thousands of Diplopoda.

Having accepted a criterion of species, the classification into higher groups is perhaps largely a matter of convenience; but convenience, scientific accuracy, and the recognition of affinities, alike demand constant attention to the fact, that the value of any character depends primarily upon its constancy, not upon the apparent *degree* of divergence. This is merely the reiteration of the chief axiom of systematic science, but the abundance of systems which completely ignore this fundamental idea are evidence that much reiteration is still desirable.

While in some natural groups it seems necessary to recognize subdivisions not definable by any constant character or complex of characters in the Diplopoda, we may conveniently proceed upon somewhat better ground, and require that the genera and larger divisions shall be limited by definite structural characters.

A dichotomous classification is theoretically the only exact one, for the reason that three or more natural groups could never be expected to be separated by exactly equivalent structural differences. Practically, however, a dichotomous system is inconvenient by reason of the great number of categories necessary in properly recognizing affinities. Hence, it is not a valid objection to the usual or multifid form of classification that the natural divisions arranged under the same category are not of the same rank, that is, not remote from each other by equal structural distances. All that can be reasonably demanded of a classification is that its groups of all ranks shall be natural ones, and that the higher the groups, the more constant, and hence fundamental, shall be the characters by which they are separated. Furthermore, it must never be supposed that the variability of a character in one group need affect its importance if found to be constant in another.

As a general policy it is evidently desirable that scientific names of all grades shall mean as much as possible. The objection to the recognition of distinct and definable genera and higher groups on account of the consequent multiplicity of names is usually to be taken as an unscientific willingness to ignore structural differences and natural affinities, in the hope of escaping additional labor. In reality the difficulty of defining groups containing unrelated members, and of becoming acquainted with such through descriptions, much exceeds the temporary inconvenience resulting from change of names.

In attempting to embody in the classification of the Diplopoda a recognition of certain structural differences found to be invariable, several natural and distinct groups of families have been recognized as orders. It is here proposed to render this classification more definite and consistent by the division of two of these orders, in the belief that the resulting groups, in addition to numerous structural differences, have long been divergent in developmental history. The orders thus to be divided are the Diplocheta and the Merocheta. From the Diplocheta it is proposed to separate the true Iulidæ and their allies, under the name ZYGOCHETA, leaving under the Diplocheta Spirostreptoidea and Cambaloidea. The Zygocheta are distinct in many characters of the gnathochilarium, in the transformation of the first pair of legs of males as clasping organs, the adnate external seminal ducts, the absence of legs from the third segment, the presence of legs on the fourth segment, and the structure of the copulatory organs of both sexes. The Diplocheta have the first pair of legs nearly or quite unmodified, the external ducts distinct, the third segment with a pair of legs, and the fourth segment footless. Notwithstanding these and other important and invariable differences, it remains probable that these two orders are more related to each other than to any third group of Diplopoda.

The other case is similar ; the Merocheta will, in the restricted sense, contain numerous families allied to the Polydesmidæ, with twenty closed segmental rings; the new order CŒLOCHETA will accommodate the

Lysiopetaloidea and Craspedosmatoidea,[3] and is characterized by the greater number of segments, the free pedigerous laminæ, the seven-jointed legs, the distinct mentum, and the normal presence of eyes. In the Merocheta the apertures of the external seminal ducts are small openings in the chitinous wall of the coxæ of the second legs, connecting with internal tube of nearly uniform diameter. In the Cœlocheta the coxæ contain a large cavity, while the aperture is large, the margin pilose and not chitinous.—O. F. COOK.

EMBRYOLOGY.[1]

The Tentacular Apparatus of Amphiuma.—In the Journal of Comparative Neurology, Vol. VI, March, 1896, Professor J. S. Kingsley has written an article entitled "On Three Points in the Nervous Anatomy of Amphibians" in which he has endeavored to show that the tentacular apparatus of Amphiuma, briefly described by me (Journal of Morphology, Vol. XI, No. 2), has been mistaken for a nerve and blood vessel. I consider the discovery of this degenerate organ of too much phylogenetic importance to be consigned at once to oblivion, and, therefore, offer in this article the results of a more careful study of it.

Since histological detail is important in this investigation, I state briefly the technique. The specimen, seventy-eight millimeters in length and seven millimeters in body diameter, was hardened in Kleinenberg's picro-sulphuric and, passed through the alcohol series from seventy to one hundred per cent and returned to seventy per cent, when the head was severed and placed three days in borax-carmine, then in acid alcohol twenty-four hours, after which it was imbedded in paraffine by the usual method and cut into serial sections one twenty-fifth of a millimeter in thickness.

Figure I is magnified twenty diameters. The outlines of all the features were drawn with a *Zeiss camera lucida*. Every feature appears in

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[3] From the true Craspedosomatidæ there may be distinguished the Trachygonidæ, Conotylidæ, and Cleidogonidæ, in addition to the Chordeumatidæ established by C. L. Koch in 1847. The separation of other equivalent groups will probably be necessary when a fuller knowledge of European and Asiatic forms is gained.

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.